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in accord with that we had indicated with larvæ.⁴ It is now clear that the old classification, employing the relative length of the palpi in the two sexes as a primary character, is not a natural one. It is true that the length of the palpi indicates, in a general way, the evolutionary state of the species. The forms with the long palpi are the more primitive ones, reduction having occurred first in the female, where, on account of the feeding habits, they were most in the way, and then in the male. The forms with the palpi shortened in both sexes are therefore evolutionally the highest.

Unfortunately for any system based on this character, the reduction of the palpi has taken place independently at different points and on this account the forms with the palpi short in both sexes can not be associated, as was formerly done. Dr. Adolpho Lutz was the first, in 1904, to subordinate the palpi and to outline a natural classification. Theobald has adopted this classification as the frame-work for his complicated scale-character system. We have found good reason to go still further than Dr. Lutz and discard the palpi for even generic limits. Aside from this it will be found, when the errors and weak points are eliminated from both, that the classification of Dr. Lutz, and the one we formulated on larvæ alone, agree very closely. Furthermore, the genitalia, when understood, indicate the same grouping. It seems that the question of stability strongly influenced Dr. Felt in his criticism. We venture to point out that only by attacking problems from new points of view can we progress. There are no fences in true science.

The further salutary effect of the study of the larval characters has been to establish a more exact and homogeneous concept of generic values. The best example is the genus *Culex*. Formerly the most heterogeneous elements were united under this name, and with those who work with the superficial characters of the imagos alone this is still the practise. Thus, even in the last volume of his work,

Theobald associated wholly unrelated forms in this genus, while, on the other hand, forms which should be included are removed and scattered through the system on account of trifling differences in the scale vestiture. We believe that an impartial and careful study of the Culicidæ from all points of view will show that we were justified in overturning the crude ideas on which their classification was based and that this was brought about through our study of the larvæ as organisms unrelated to their adults. We believe that the names founded on larvæ are valid and should be given due priority. The characters used are in many cases more positive and reliable than those found in the adults and are further of more evolutionary importance than those heretofore used in classifying the adults.

HARRISON G. DYAR
FREDERICK KNAB

THE GERM-CELL DETERMINANTS OF CHRYSOMELID EGGS

THERE lately appeared in these columns under a title similar to the above, an article¹ by Dr. R. W. Hegner dealing with a paper of mine² in which I had questioned his use of the term "germ-cell determinants" as applied to the granules of the pole disc of chrysomelid eggs. As I do not quite agree with Hegner's interpretation of my position, I take this opportunity to make a more explicit statement of my point of view.

To my mind, a cell or tissue *determinant* suggests or implies a physiological activity that, to use a crude illustration, resembles the physical action of a dye upon impressionable metal. Thus a germ-cell determinant would be something that stamps the undifferentiated cells arising from the cleavage nuclei with a specific germinal or reproductive property.

I have given evidence that what he calls "germ-cell determinants" are in all likelihood merely a part of the food stream from the nurse cells, and this Hegner admits is no

⁴ "On the Classification of the Mosquitoes," *Canad. Entom.*, Vol. 39, 1907, pp. 47-50.

¹ R. W. Hegner, "The Germ-cell Determinants in the Eggs of Chrysomelid Beetles," *SCIENCE*, Vol. XXXIII., No. 837.

² *Biol. Bull.*, Vol. XVIII., No. 4.

doubt correct. As far as I have been able to determine, when they first enter the egg, these granules differ in no wise from those in other parts of the food stream that develop into yolk spherules.

Now the early developmental features of the germ cells in this species point not so much to a process of active differentiation as one of passive isolation, which results in the pole cells retaining or preserving the reproductive potentialities of the cleavage nuclei; the pole-disc meanwhile serving as food material for the pole cells which "as a result of this special kind of nutrition undergo a peculiar method of metabolism which differentiates them from the somatic cells"—just as a certain kind of food is necessary for the early growth and development of a child, but is by no means the cause of its becoming a man instead of an ape.

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January 27, 1911

THE PYTHAGOREAN THEOREM

DR. NORTHRUP'S article¹ is not a proof of what is sometimes—perhaps incorrectly—called the *Pons asinorum*, unless it be shown *experimentally* that the kinetic energy of a body is the sum of its energy of translation and of rotation. The deduction, however, of this theorem of energy from the fundamental propositions of mechanics depends on the law of vector superposition, the mathematical expression of which involves the Pythagorean theorem. In general it is not economical to derive mathematical propositions from experimental physics; moreover, the process fails to bring out that difference between mathematics and physics which is shown, for example, in Hilbert's "Foundations of Geometry" and Mach's "Science of Mechanics."

I should like to be permitted the liberty of objecting to the statement:²

"No motion, force or acceleration which exists at the point p can produce rotation of 1—2 about p as center. This must be so, as it is axiomatic in dynamics that, when there is

a force or acceleration at the center of mass only of a body, there remains no couple to produce rotation": first, because the word "axiomatic" seems to be used in the Kantian sense of "self-evident," and second, because Dr. Northrup's proof (?) in no way depends on whether p has linear or 1—2 has angular acceleration.

Equation 7 of the paper expresses a geometric fact—I am tempted to say "accident"—which text-books raise to the dignity of a theorem.

R. F. DEIMEL

TO THE EDITOR OF SCIENCE: Referring to your December 16 issue, if we are to have "A Dynamical Proof of the Pythagorean Theorem," why not let it be a simple one? For instance, if the force F whose rectangular components are X and Y , acts upon a particle of mass m until it has imparted the velocity q whose components in the same plane are u and v , then the work done upon the particle by X is equal to $\frac{1}{2}mu^2$, while the work done by Y is $\frac{1}{2}mv^2$. But the work done by the components is identical with the work $\frac{1}{2}mq^2$ done by their resultant. Equating and cancelling the factor $\frac{1}{2}m$,

$$q^2 = u^2 + v^2.$$

But the velocity components u and v are the two legs of a right triangle of which q is the hypotenuse, so that here again is our Pythagorean relation.

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QUOTATIONS

THE TENURE OF PROFESSORSHIPS

AMONG the reforms suggested by the "efficiency expert" of the Carnegie Foundation who investigated the administration of some of the principal American universities was the appointment of professors for a brief period, so that they could be dropped without fuss whenever for any reason a change was desired. His idea was to get young, vigorous men, work them hard as long as they could stand the strain, and then pension them off in the interest of efficiency. Somewhat similar views have of late been expressed by several univer-

¹ SCIENCE, XXXII., 833, p. 864.

² L. c., p. 864.